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York University on April 22d the corner stone of the first residence hall was laid. The building, which will be ready for use in September, will contain, in addition to rooms for 112 students, a music room, editorial rooms for the college periodicals, etc.

PROF. GEORGE S. FULLERTON, Vice-Provost of the University of Pennsylvania and Dean of the College, will retire from the latter office and will be succeeded by Prof. W. A. Lambertson, who in turn will be succeeded in the deanship of the School of Philosophy by Prof. W. R. Newbold.

DR. ERNEST B. SANGREE, of Philadelphia, has been elected professor of pathology and bacteriology in the Vanderbilt University, Nashville, Tenn.

DISCUSSION AND CORRESPONDENCE.

ON ROOD'S DEMONSTRATION OF THE REGULAR OR SPECULAR REFLECTION OF THE RÖNTGEN RAYS BY A PLATINUM MIRROR.

ON March 27th Prof. Rood published in this JOURNAL a short account of certain experiments which he claimed 'pointed strongly to the conclusion that in the act of reflection from a metallic surface the Röntgen Rays behaved like ordinary light.' If this sentence means anything, it means that the X-rays underwent regular or specular reflection. On April 10th, however, Dr. M. I. Pupin published in this JOURNAL an article in which he says, "If I understand Prof. Rood's words correctly, no claim is made by him of a discovery of regular or specular reflection;" and he then quotes from Rood the sentence given above. The remainder of Dr. Pupin's article is largely devoted to showing that with the methods employed by him no regular or specular reflection could be observed. This last conclusion we are ready to accept. Prof. Rood's experiments, however, were conducted in an entirely different manner, as follows:

Before reaching the sensitive plate the X-rays were obliged to traverse two aluminium plates, each having a thickness of .17mm., and behind them was a drawslide that had proved to be impervious to the sun's light falling on it during two hours. Over these shields was placed

a wire netting with openings of $\frac{1}{8}$ inch. The reflecting surface was a large piece of bright platinum foil, seven inches square. This last was necessarily so arranged that a diffused reflection from it would have reached all parts of the sensitive plate. In point of fact, however, an image of the wire netting was obtained only on a strip of the plate, viz., on that portion that would be reached by the Röntgen rays in case of their regular or specular reflection.

The proof that the image of the wire netting on the sensitive plate was really produced by the *specular reflection* of the X-rays from the platinum was obtained in the following manner. The plate which had received the image of the netting made by the X-rays was removed from the plateholder and replaced by a fresh plate; this plate was not screened at all, but its sensitive surface was freely exposed in the dark at night.

Everything else in the arrangement of the experiment, including the position of the netting in front of the plate, remained as it was during the experiment with the X-rays. One flash from the inductorium was sent into the Crookes tube and the experiment was ended. On developing the plate it was found that *the light* from the Crookes tube had exactly reproduced in a fraction of a second what had required ten hours of action of the X-rays. There was the same portion of the plate acted on by the light as had been acted on by the X-rays, and the image of the netting given by the X-rays was reproduced by the light, not generally reproduced but minutely so; all the deformations of the image of the netting resulting from the reflection from the uneven surface of the platinum foil were alike in the photograph obtained by the X-rays and in the photograph obtained by the light.

I paid repeated visits to Rood's laboratory during the progress of these experiments, and after a careful examination of his negatives no doubt remained in my mind of the fact that he had demonstrated the regular or specular reflection of the Röntgen rays.

Prof. Rood carried these and other similar negatives to Washington, where he read a paper on the reflection of the X-rays before the National Academy of Science on April 23d.

The original negatives were carefully examined by the physicists present, Prof. Rowland devoting half an hour to their critical examination; and I do not think that the slightest doubt was held, by any of the six physicists present, of the completeness of the demonstration.

I cannot conceive how Dr. Pupin, after an examination of Rood's photographs, could differ from, deny, or even doubt, the conclusions reached by several of the most critical and experienced physicists of the country after their examination of these photographs.

ALFRED M. MAYER.

PSEUDO-SCIENCE IN METEOROLOGY.

IN the issue of SCIENCE for April 10th nearly a full dozen columns of valuable space have been devoted (under a rather misleading title) to recording observations and opinions which are to prove the absence of a favorable influence of forest cover on meteorological phenomena and especially on waterflow in the Western Mountains.

Since this subject has become not only one of considerable scientific interest, but also of great national importance, far-reaching economic policies depending in part on the answer which science or well sustained observation and argument can give to the question, it may not be out of place to devote further space to the question in order to warn against the many erroneous observations and fallacious conclusions contained in the article referred to.

I do not wish to offend the writer when I say that by neglecting to sift more carefully the untutored and too-often-prejudiced opinions and notions of so-called 'practical' men at the hand of the *established* facts of science, physical, physiological and meteorological, he has done harm; for he has not only increased the accumulations of 'practical' or pseudo-science, to which to be sure, many so-called 'scientists' contribute no small share, but he has also discredited the sometimes valuable—when used with discretion—observations of laymen with those men of science who read with a knowledge of the laws of physics and the facts of meteorology before them.

Sure enough meteorology, especially on the

side of accurate measurements, is but poorly developed; nevertheless there is much more real knowledge in existence regarding many of the physical processes and conditions involved, not only qualitatively, but even quantitatively (as, for instance, regarding the behavior of snows, the evaporation of water, the transpiring of trees, and the conditions which influence these and the run-off and waterflow of rivers) than the writer of the article is aware of, so that it is not necessary to rely on *opinions* of 'practical' observers for these details at least.

I wish, however, not to be understood as discrediting in any way field observations and argument from them and as insisting upon accurate measurements as the only basis for the explanation of natural phenomena. On the contrary, I am one of those who believe that many complicated natural phenomena withdraw themselves for the present, *i. e.*, with our present knowledge and means, from accurate measurement; being results of complex and variable conditions which we are not prepared to measure, we may only by careful, long continued field observation and upon sound argument from well-known physical laws come to conclusions and determine relations qualitatively, leaving quantitative measure of these relations to be worked out in the future with improved method.

The present question, namely, that of forest influences on meteorological phenomena, is one of these, for in the first place we have as yet neither instruments nor methods to measure with any determinable degree of accuracy the rainfall over a given area, much less the evaporation; and even riverflow is not yet satisfactorily measured. And when it comes to the many varying influences affecting these phenomena quantitatively, we are entirely debarred from speaking with assurance even as to methods of determining them.

It would require too much space to discuss in detail the many erroneous statements and conclusions contained in the article referred to and which any meteorologist or physicist can readily discover. I shall have to confine myself to pointing out the fallacy of the main argument, which appears the more important as it has been advanced before by others with a flavor